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#### DISCUSSION AND CORRESPONDENCE

##### M. COSSMANN ON THE PHYLOGENY OF CERITHIUM

IN the *Revue Critique de Paléozoologie* for April, 1911, M. Cossmann published a review of my paper on "The Phylogeny of Certain Cerithiidae" which involves a question of fundamental principles in the study of phylogeny and is therefore of interest to consider at greater length.

M. Cossmann calls attention to the fact that my classification differs widely from that published in his monograph on the Cerithiidae.<sup>1</sup> The reason for this is, as stated in my paper, that we are following entirely different methods of work. We are, I think, in accord in assuming that a natural classification should be based upon descent from a common ancestor but as to the principles to be followed in determining relationship we differ widely. M. Cossmann's classification is based on a comparison of the aperture and especially of the "cerithial" canal, mine upon the entire ontogeny of the shell, the facts thus obtained being applied in accordance with Haeckel's biogenetic law. M. Cossmann's argument in favor of using the aperture as a basis of classification is stated in his review as follows: "C'est par l'ouverture que sortent les organes d'un Gastropode, c'est par là que son manteau secrète le test; c'est donc l'ouverture qui joue le principal rôle dans l'évolution." It is true that the mantle secretes the shell at the aperture, but if the adult aperture be considered of so much importance how can we neglect the succession of apertures represented by the young

shell every growth line of which outlines the aperture of the shell at the time when the line was formed. If it be true, as stated by Hyatt,<sup>2</sup> that "All modifications and variations in progressive series tend to appear first in the adolescent or adult stages of growth" we shall find in the adult aperture the extreme limit of variation for the individual, and it is to this stage that we look for divergence from the well established, hereditary characters that ally the organism with its ancestors. If recapitulation be a fact it is in the young stages that inherited characters find their fullest expression. A defense of the methods used in my paper and of my results is simply a defense of the theory of recapitulation, and no adequate presentation of the subject can be attempted in a limited space. The validity of the theory is still questioned by some scientists, mainly zoologists, but the final answer to the question will be, as in the case of the theory of evolution itself, an accumulation of corroborative facts so overwhelming as to finally silence doubt. Already the accumulation of such facts is so considerable as to convince nearly all paleontologists and many zoologists. In an excellent summary of the present status of opinion on this subject Cumings<sup>3</sup> has called attention to illustrations of recapitulation in each of the classes of invertebrates above the Porifera.

Against this mass of evidence a mere dogmatic statement has little weight. It is not enough to cry scornfully, "Quelle importance peut-on attribuer à des conclusions basées sur de telles prémisses?" He who would show these premises to be unsound must show Haeckel's law to be invalid by answering the arguments of Hyatt, Cope, Jackson, Beecher, Cumings and many others, and also otherwise account for the great accumulation of evidence in favor of the law which appears not only in the works of authors avowedly in favor of the theory but in the facts presented

<sup>2</sup> A. Hyatt, "Genesis of the Arietidae," p. ix, Smith's Cont. to Knowledge, No. 673, 1889.

<sup>3</sup> E. R. Cumings, "Paleontology and the Recapitulation Theory," *Proc. Ind. Acad. Sci.*, 25th anniversary meeting, 1909.

<sup>1</sup> M. Cossmann, "Essais de Paleoonchologie Comparée," VII., July, 1906.

by many other workers whether they call attention to the recapitulation shown or not.

As a whole M. Cossmann's criticism shows a total misconception of modern methods in phylogenetic study and even the illustrations which I have used to point out likeness or difference in descent are to him, judging as he does by the standards of the older conchologists, only so many offences against the good old fashioned rule of putting together species that are alike in the adult and ignoring "simples et légères modifications dans l'ornamentation de la spire." A careful comparison of the detailed figures in my paper will, however, show that the modifications are not slight in cases where genera are separated.

As might be expected, the choice of a genotype from the work of a pre-Linnean author is questioned. This choice arose from the difficulty of applying the established rules of nomenclature in such a manner as to meet the approval of all students of the subject. Bruguière, the first post-Linnæan author to use the binomial nomenclature in connection with *Cerithium*, did not select a genotype, and Lamarck chose, at different times, two of Bruguière's species as illustrations of the genus. At present one eminent authority chooses as genotype the first of Lamarck's selected species, while another chooses the second and a third suggests a choice from Bruguière's many species. If the general consensus of opinion finally fixes upon *Pseudovertagus aluco* or *Cerithium? nodulosum* instead of *C. tuberosum* as the type of *Cerithium* it would simply necessitate the choice of a new name for the group represented by *C. tuberosum*, *C. adansonii*, etc., which were the first to be described, and which have long been known by that name. However, the name applied to a natural group is of minor importance. The point of especial importance is that such shells as *Pseudovertagus aluco* and *Cerithium echinatum*, for example, can not be classed together in the same genus since their ontogeny shows that they have an entirely different ancestry. The main object of my paper is to trace the phylogeny of a natural group and to show the methods by

which relationship should be determined.

In summarizing I would emphasize the following three points:

1. A natural classification should be based on community of descent.
2. In tracing descent the whole ontogeny is a more reliable guide than a single final stage of it.
3. There is sufficient evidence in favor of the theory that ontogeny recapitulates phylogeny to make it the only safe means of determining relationship.

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#### A NEW RACK FOR INDIVIDUAL TOWELS

THE "common towel" is a problem which sanitarians so far have failed to solve. This is especially true of the roller towel so often found in public places. The fact that these towels are dirty and unattractive, if not repulsive, is comparatively unimportant, for if the real facts were known it would be understood that many cases of disease are transmitted by this means. In the better places where the wash room can have frequent attention, small individual towels can be used. In a good many places, however, it seems impossible on account of the expense, due to those lost and stolen. One attempt to solve the problem has been the substitution of paper for cloth towels. In the minds of most people, however, these paper towels are not satisfactory, although, of course, they are perfectly hygienic.

An attempt has been made at the University of Wisconsin to solve this problem and towel racks have been designed which make it possible for each person to have an individual towel. This rack is very simple, as is seen in the accompanying illustration. It consists essentially of a shelf on which are placed a pile of the small, clean towels, and just below this and at one side is a receptacle or basket into which the dirty towels are placed. Connecting the shelf and the basket is a rod with a goose neck. The towels are provided with a perforation or loop through